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ORIGINAL PAPER

A Quasi-Experimental Evaluation of the Impact of Public Assistance on Prisoner Recidivism

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Abstract
Introduction The Welfare Act of 1996 banned welfare and food stamp eligibility for felony drug offenders and gave states the ability to modify their use of the law. Today, many states are revisiting their use of this ban, searching for ways to decrease the size of their prison populations; however, there are no empirical assessments of how this ban has affected prison populations and recidivism among drug offenders. Moreover, there are no causal investigations whatsoever to demonstrate whether welfare or food stamp benefits impact recidivism at all.
Objective This paper provides the first empirical examination of the causal relationship between recidivism and welfare and food stamp benefits
Methods Using a survival-based estimation, we estimated the impact of benefits on the recidivism of drug-offending populations using data from the National Corrections Reporting Program. We modeled this impact using a difference-in-difference estimator within a regression discontinuity framework.
Results Results of this analysis are conclusive; we find no evidence that drug offending populations as a group were adversely or positively impacted by the ban overall. Results apply to both male and female populations and are robust to several sensitivity tests. Results also suggest the possibility that impacts significantly vary over time-at-risk, despite a zero net effect.
Conclusion Overall, we show that the initial passage of the drug felony ban had no measurable large-scale impacts on recidivism among male or female drug offenders. We conclude that the state initiatives to remove or modify the ban, regardless of whether they

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improve lives of individual offenders, will likely have no appreciable impact on prison systems.

Keywords Welfare · Food stamps · Drugs · Ban · Prison · Recidivism

Introduction

In response to the growing financial and social pressures of mass incarceration, policy-makers are evaluating policies and practices in the criminal justice system and searching for ways to reduce correctional burden while protecting the public interest. One policy that has drawn recent attention is the drug felony ban on food stamp benefits (now called the Supplemental Nutrition Assistance Program or SNAP) and cash assistance (known as Temporary Assistance to Needy Families or TANF). Originally introduced in 1996 as part of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), this ban completely denied SNAP and TANF eligibility for "individual(s) convicted (under federal or state law) of any offense which is classified as a felony… and which has as an element the possession, use, or distribution of a controlled substance."

At the time it was passed, proponents of the ban criticized drug felons for receiving public benefits despite having broken the nation's drug laws and argued for denial of benefits on the basis of moral and social principles (Godsoe 1998; Allard 2002).[1] In years since, critics of the ban have argued that denying benefits creates a net harm to society, worsening outcomes for needy populations and especially for women and children (Mauer and McCalmont 2013; Godsoe 1998; Allard 2002; Eadler 2011). Importantly, the original law gave states the ability to opt out or modify their use of the ban through legislative reforms.

This feature is important because it suggests why legislators still care about the ban today; states across the country are increasingly viewing removal of the ban as a way to reduce the number of drug offenders returning to prison after they are released. For example in 2014 and 2015, Missouri and California (respectively) enacted new laws that completely or partially removed the SNAP ban for convicted drug felons. Similarly, in 2015 the Alabama legislature passed a prison reform bill that allows drug felons to start receiving benefits in 2016 (Edgemon 2015). These illustrations are telling—the high costs of prisons and changes in social and political attitudes towards the ban are driving its re-examination.

Despite the political rhetoric surrounding the use of the ban, there is no direct empirical evidence to support or reject whether states can measurably affect prisoner recidivism or the size of prison populations through their use of the ban. In fact, there does not appear to be any causal evidence whatsoever to demonstrate that the receipt of TANF or SNAP benefits does or does not have an impact on an individual's propensity to return to prison.

This paper investigates the relationship between receipt of public assistance (specifically, in the form of SNAP and TANF benefits) and recidivism by examining how the enactment of the drug felony ban impacted recidivism rates for drug offending populations. Using individual-level prison records from the National Corrections Reporting Program (NCRP) across six states, we estimated the impact of the ban's 1996 implementation on

[1] In fact, the ban itself was a relatively obscure provision in a much larger piece of legislation. Congressional records show that the ban provision saw <2 min of total debate (Mauer 2002; Petersilia 2003).

rates of returning to prison. We defined a return to prison as a return for any reason (conviction or revocation) and for any type of crime.[2] Impacts were identified using difference-in-difference estimation within a regression discontinuity framework, and were estimated through survival-based regression modeling techniques (i.e., proportional hazards models) described in subsequent sections.

Overall we find no strong evidence to support the claim that recidivism rates or the size of prison populations has been materially influenced by the drug felony ban. Among both male and female prison populations, the estimated pooled impact of the ban is not statistically different from zero (with point estimates very near zero). Across states, estimates are more variable; however, for both male and female prisoners, state estimates provide no consistent depiction of how these populations are affected by policy changes.

Results are also extremely robust to alternative model specifications. We test the sensitivity of our results to more flexible time trends and alternative parametric specifications and find no meaningful changes to baseline results. This implies that changes to drug felony ban implementation cannot materially influence the size of prison populations in the aggregate.

We discuss potential explanations for this null finding later in the paper. One of those possible explanations, which we explore empirically, is that impacts may be heterogonous with respect to time-at-risk. If true, then local average treatment effects could be zero while treatment effects within the sample vary. We tested this by stratifying estimates by time-at-risk (using 6- and 18-month intervals). From this test we find evidence suggesting that denying benefits may in fact improve short-term outcomes while worsening long-term outcomes. At the very least, we see this evidence as motivation for future study.

The remainder of this paper is organized as follows. First, we present a background discussion on the role of public assistance in re-entry and features of the drug felony ban. Next, we describe the data we use for this analysis and our methods for identifying impacts, followed by a presentation of results. We conclude with a discussion of the limitations of our analysis and closing remarks.

Background

Offender Re-Entry, Economic Challenges and Use of Welfare

There is a large body of research devoted to understanding how offender outcomes are shaped by the economic challenges they face after prison (e.g., Western et al. 2014; Travis 2005; Petersilia 2003). The reason is that offenders, like other low-income populations, are economically disadvantaged and in need of services that can mitigate barriers to successful re-entry. Employment is one of the most oft-studied outcomes (e.g., Kling 2006; Bushway et al. 2007; Stoll and Bushway 2008), though other economic considerations such as housing, court-imposed sanctions (fines, restitution and fees), use of public assistance and demand for health services also receive significant attention in the literature (e.g., Sheely and Kneipp 2015; Lindquist et al. 2009; Evans 2014; Geller and Curtis 2011).

[2] Since the NCRP does not capture alternative measures of recidivism (e.g., rearrest, reconviction, incarceration in jail, etc), we could not explore alternative definitions in our analysis. However, return to prison is a useful and important measure (e.g., Hunt and Dumville 2016; Langen and Levin 2002; Durose et al. 2014). It is often used as a metric for evaluating programs, assessing trends and gauging impacts for other correctional issues of interest, often in concert with other metrics such as rearrest or reconviction (e.g., Bales et al. 2005; Spivak and Damphousse 2006; Steurer and Smith 2003).

Two specific public assistance programs, SNAP and TANF, provide significant supports to low-income households and families in general, though their use among offending populations in particular is unclear. On a national scale, benefits paid by SNAP each month in FY2014 averaged roughly 5.8 billion dollars over 46 million individuals, or $125 per person per month (US Department of Agriculture 2017). For TANF, FY2014 benefits paid each month averaged $2.6 billion dollars (including both federal and required state spending) over 3.9 million recipients, or around $667 a month (US Department of Health and Human Services 2016). This level of support suggests that both programs may provide an important level of assistance to offenders as they re-enter the community. In addition to simple subsidy support, TANF assistance can also include a variety of services that may further promote successful reintegration such as job training, counseling and crisis management.

Despite its likely importance to offenders, receipt of public assistance and its impact on re-offending in the post-release period is an issue we know surprisingly little about. This is not because the issue is unimportant or has been overlooked. Rather, there is a fundamental lack of data sufficient to study the issue. Few data sources exist which tie together welfare receipt and longitudinal outcomes with incarceration, criminal history, and other criminal measures (Sheely and Kneipp 2015; Butcher and LaLonde 2006; Holtfreter et al. 2004).

Even the most basic statistics are difficult to find. For example, we were unable to locate any national estimates of how many released offenders receive public assistance including SNAP or TANF.[3] Overall, the limit to our knowledge at present appears to be this: likely somewhere between 25 and 40% of female prisoners are eligible for SNAP and/or TANF after release; for males this number is likely between 10 and 20% (Lindquist et al. 2009; Lattimore et al. 2009; Ekstrand 2005; Allard 2002; Butcher and LaLonde 2006; Hirsch 1999). These estimates are both crude and imprecise. They are also evolving as we learn more. For example, a recent longitudinal study of prisoners released in Boston suggests the likelihood of receiving benefits increases significantly over time, and that welfare receipt in the post-incarceration period may be as high as 70% (Western et al. 2014).

Despite the general lack of empirical data on SNAP/TANF participation and program impacts for offending populations, there are many studies that have examined program impacts on employment, household structure and household earnings, housing and food security and health for participants more broadly (e.g., Blank 2002; Schoeni and Blank 2000; Lindner and Nichols 2012; Bitler 2014). Evidence from this literature suggests that programs like SNAP and TANF can and do have positive impacts on the lives of individuals in many cases. In that case, it seems reasonable to assume that offending populations enjoy similar benefits from participation. For these reasons, scholars have argued that "an offender's eligibility to receive public assistance is critical to successful reintegration" (Petersilia 2003).

SNAP, TANF and Recidivism: The Potential Impact of Denying Benefits

Despite the intuitive appeal of the argument, "benefits should improve offender outcomes and thereby reduce recidivism," there is no direct, causal evidence to support or refute this claim. If benefits extend the affordability of basic needs and services like food, housing, drug treatment, physical and mental heath services, etc. (Allard 2002; Mohan and Lower-

[3] The closest source to a nationally representative picture we could locate comes from the Bureau of Justice Statistics Inmate Survey, which provides limited information on welfare receipt before an arrest and during an offender's childhood. This survey does not track offenders over time.

Basch 2014; Mauer and McCalmont 2013; Godsoe 1998), then providing benefits should reduce the need for (and causes of) criminal behavior, thereby decreasing the likelihood of reoffending (Petersilia 2003). At least some empirical research supports such associations between poverty, state supports and recidivism (Holtfreter et al. 2004).

On the other hand, benefits may also be counterproductive as a means of reducing recidivism, particularly in the case of drug offenders. One possibility is that benefits provide drug users with additional purchasing power that allows them to substitute purchases of other goods for more drugs (Johnson et al. 1985). If more income leads to greater drug use, providing benefits may serve to increase recidivism rates among beneficiaries. Alternatively, recipients may fraudulently trade their benefits for drugs or for cash used to purchase drugs (Roebuck 2014; Statement of the Honorable Phyllis K. Fong Inspector General 2012; Oregon Revised Statute §411.119 2005).[4] Receipt of benefits could also reduce the pressures to engage in other prosocial behaviors during the post-release period, e.g., consistent job-seeking or more frequent visitation with supervision officers.

Another consideration is that SNAP and TANF programs serve different (but overlapping) populations, such that their potential importance to offenders and ultimately corrections systems should also vary along these dimensions. For example, the proportion of adult males receiving SNAP (around 44% of adult participants) is much higher than for TANF (around 15% of adult participants) (US Department of Agriculture 2017; US Department of Health and Human Services 2016). This implies that changes pertaining to SNAP are more likely to have the greatest impact on prisons, where males make up the majority of inmates. Conversely, female prison populations would be more impacted by restrictions to TANF. Such variations help to explain potential differences in impacts we might find between men and women.

As another example, consider that nearly 20% of SNAP households are nondisabled, childless adult households, while only 6% of TANF households are single-member households. If offenders tend to be young individuals without children, then understanding how SNAP benefits can affect outcomes becomes more relevant to understanding how the ban may or may not affect change. Such nuances are critical to understanding how programs may (or may not) translate to the impacts we test for in our analysis.

Using the Ban as a Natural Experiment for Denying Benefits

The goal of this paper is to test these competing theories using state variation in implementation of the drug felony ban as a natural experiment. Specifically, our goal is to determine whether changes to the drug felony ban led to material changes in the rate of recidivism for the prison population of drug offenders. To do this, we tested the impact of the ban by looking at differences in recidivism for offenders convicted before and after the ban's initial adoption. Earlier iterations of this paper also considered whether interim changes (i.e., modifications) to the ban's application impacted offender outcomes. However, because these changes occur on the basis of calendar date rather than conviction date, the strength of our identification is arguably weaker and results are less informative. As a result we have excluded these analyses from the paper. Nevertheless it can be noted that results from these additional analyses were consistent with the findings of this paper.

[4] In fact, there is explicit mention of trading benefits for drugs and the associated penalties in the SNAP benefit application form in Louisiana. (http://www.dcfs.louisiana.gov/assets/docs/searchable/EconomicStability/Applications/OFS4_4I.pdf).

Across 10 states where we tested impacts for men and women (16 tests altogether), none showed significant changes resulting from ban modification.

Later sections describe the data and methods we used for this analysis in greater detail; however, an important, upfront acknowledgement is that our data do not allow us to identify individual eligibility (or receipt) of benefits for specific offenders. Thus we cannot estimate the ban's impact as it affected specifically those whose eligibility was altered or denied by the ban. Instead, we estimate the impact of the ban as it was "assigned" (by its passage) to all offenders, regardless of eligibility. In the parlance of statistical evaluation, our estimated treatment effect is modeled using an "intent-to-treat" (ITT) framework, rather than as an estimate of the "treatment on the treated" (TOT) (Angrist 2006). Nevertheless our investigation does inform an important policy-level consideration: Can removal or modification of the ban reduce the size of the prison population? Will it result in savings for corrections agencies?

Such questions are even more important when considering whether the ban ever led to actual changes in the practices it was meant to influence in the first place. For example, Butcher and LaLonde (2006) show that in Cook County, Illinois, bans on TANF receipt did not significantly affect attachment to the welfare system for drug felons.[5] Whatever the reason, such a finding implies that removal of the ban will have no impact since, as it is designed, it does not achieve its primary goal of denying benefits. In cases such as this, the question of "do benefits matter?" is secondary to the policy concern, "does the ban work?" Our ITT analysis informs a question much like the latter—"does the ban create meaningful system-level change?"

State Implementation of the Drug Felony Ban

Since the PRWORA became law, states have varied considerably in their response to the ban and the timing of that response. Within 18 months of PRWORA enactment, 4 states had opted out of the ban entirely; today that number has grown to 14.[6] Twenty-six states have modified the ban to allow benefits, subject to additional requirements imposed on drug felons specifically. Ten states have not altered their use of the ban at all. Though we could not confirm the status of Wyoming's laws, best indications are that Wyoming has a full ban in place.

States' initial adoption of the ban can be classified as one of three types of changes: (1) moving from no ban to a full ban, (2) moving from no ban to a partial ban, or (3) opting out immediately. One state with available data opted out immediately: New York.

The meanings of *full ban* and *no ban* are clear: full ban implies total adoption of the PRWORA provision (i.e., felony drug offenders are completely barred from receiving SNAP or TANF benefits) and no ban implies no ban was in place (i.e., felony drug offenders do not face special conditions). The meaning of *partial ban* is more ambiguous. States with partial bans impose at least some special conditions for eligibility, and in

[5] The analysis of Butcher and LaLonde raises an interesting question of whether state agencies are in fact complying with the federal law. Though we cannot say with absolute certainty that every state complies, evidence gathered for this research (e.g., SNAP application forms asking about drug conviction status, and a conversation with a Massachusetts congressional representative) suggests that policies have resulted in operational changes at the agency level. (http://www.dcfs.louisiana.gov/assets/docs/searchable/EconomicStability/Applications/OFS4_4I.pdf).

[6] Gabor and Botsko (1998) report that 10 states opted out of the ban on food stamps in the year following the PRWORA ban. Those results were based on a survey of states and only report responses for the food stamp portion of the ban. Our independent research has led us to conclude that only 4 states had fully opted out of both aspects of the ban (i.e. completely removed restrictions to both SNAP and TANF).

practice, these conditions can vary considerably across states.[7] For example in Iowa, drug felons are only eligible for benefits if they participate in drug treatment. In Louisiana, drug felons only become eligible one year after their release. In Florida, drug felons convicted of possession are eligible, while those convicted of trafficking are not. Given the heterogeneity within states' use of partial bans, we do not to attempt to tease out impacts of various forms of partial restrictions. That is to say that we do not attempt to measure differential impacts between, e.g., "random drug testing" and "required drug treatment."

Finally, it should be noted that while the PRWORA itself denied benefits to all offenders for SNAP and TANF simultaneously, modifications have sometimes addressed these programs separately, in both substance and timing. For example, Washington first removed the ban on SNAP benefits in October 2004, then removed the ban on TANF benefits almost a year later, in September 2005. Changes of this nature are the exception rather than the rule; most states have modified both SNAP and TANF eligibility requirements at the same time (Ekstrand 2005).

Data

For this study, we combined prison data, legislative data and county-level data compiled by the US Census to construct a single analytic dataset. Prison data come from the National Corrections Reporting Program (NCRP)—an annual data collection program (operated by the Bureau of Justice Statistics) that collects prison admission and release data for individual offenders in every state across the US These offender-level data include information on offender characteristics such as sex, age and race, and sentence information such as offense type, time spent in prison and sentence length.

Though NCRP data go back as far as 1983, known issues with data reliability make much of the early data problematic (Rhodes et al. 2012; Neal and Rick 2014; Pfaff 2011). More recently, NCRP data collection and assembly have been redesigned to provide more reliable information (Rhodes et al. 2012). Data are now constructed as longitudinal, panel datasets (called "term files") tracking individual offenders and their movements into and out of prison over a given reporting period (Luallen et al. 2012). Reporting periods covered in the NCRP data vary from state to state, with the most common window beginning in January 2000 and extending to December 2014.

There are only six states in the NCRP with data extending back to 1996 where impact estimates are possible: California, Florida, Georgia, Illinois, Michigan, and Minnesota.[8] Given that our interest is in analyzing the impact of the ban when it was first passed in 1996, only these states can provide an unbiased sample of offenders who entered prison during that time.

We also assembled legislative data on a state-by-state basis so that we could control for state-level changes in ban use over time. We compiled this data using multiple sources. One source was the "State Options Reports" published by the Food and Nutrition Service (FNS) (US Department of Agriculture 2016). These survey-based reports provide high-

7 Broadly, states adopt three types of partial reforms: (1) requirements for offenders to participate in or complete treatment before receiving benefits; (2) allowance for drug offenders who committed less serious crimes to access benefits; and (3) allowance for offenders to receive benefits after a probationary period following release.

8 New York has data back going back to 1994, but opted out immediately after the ban was passed. We separately tested our pooled estimation with and without New York and found no difference in findings between models.

level summaries of each state's policies regarding the drug ban and modifications thereof. They extend back to 2002 and are typically published once every one to two years. We augmented these reports with independent web searches and queries in a legal database (Westlaw). In a number of cases our search results conflicted with the FNS reports.[9] In those cases, we disregarded the FNS survey data in favor of source documents.

Table 1 below provides a summary of relevant state laws and NCRP reporting windows for all 50 states. Though our sample used only a subset of these states, the complete table provides a useful resource for researchers. It does not document every legal change that has occurred over time; rather, it describes major policy shifts as defined earlier in this paper.

Finally, we supplemented these data with county-level information compiled by the US Census Bureau. These data include county-level descriptions of population density, economic conditions (such as poverty rates and household income), education level and SNAP participation rates. Most of these data are made available through Census's USA counties data products, though some information (including rates of SNAP recipiency) is reported as part of Census's intercensal estimates.

Method

To estimate the impact of the ban, we combined two popular inferential methods for estimating causal effects: regression discontinuity (RD) design and difference-in-differences (DiD) estimation. Our use of RD design provides defensible measures of causal impacts by minimizing observed and unobserved differences between comparison groups. Our use of second-differencing (DiD) strengthens the credibility of these results by controlling for other possible coincident, exogenous shocks that may also have impacted recidivism but were not the result of the ban. We explain our use of each.

The motivation for our quasi-experimental approach is straightforward. Consider first a simple approach that estimates ban impacts as the unadjusted pre-post comparison between treated and untreated groups (in this case, average outcomes before vs. after the ban). In order for estimates to be unbiased, before and after groups must be characteristically equivalent with respect to measures correlated with the outcome. That condition is unlikely to hold without adjustment; however, even with adjusted comparisons one cannot reject that possibility that unobserved group differences correlated with the outcome still exist. The problem worsens when unobserved differences are changing (or trending) in the pre and post periods. Quasi-experimental methods can overcome such limitations and, in the context of our analysis, we use RD to do this.

RD designs operate under a simple premise: unbiased treatment effects can be identified when the probability of treatment is a discontinuous function of one or more underlying measures (Imbens and Lemieux 2008; Cameron and Trivedi 2005), also called forcing variables. Discontinuities occur at specific thresholds (or cutoffs), such that treatment assignment depends (discontinuously) on whether individuals fall above or below the cutoff. By extension, when individuals have imprecise control over the assignment to treatment, treatment–control comparisons in a local neighborhood around the cutoff can be analyzed like randomized experiments (Lee and Lemieux 2010). That is to say that nearby

[9] Apparent confusion by states as to what is meant by "ban modification" has led to reporting error in the State Options Report, and subsequently, confusion in the literature as to what states have adopted what policies and when. For example, although Iowa imposes some drug rehabilitation services (or other requirements) for former drug felons, FNS reports show it has opted out since 2006.

Table 1 (a) List of ban modification statutes and enactment dates identified for analysis, (b) dates and statutes ban modifications used in analysis

State	Modification 1			Modification 2			NCRP	
	Type	Date	Bill/law	Type	Date	Bill/law	Start	End
(*a*)								
Alabama	None	NA	–	NA	NA	–	2007	2014
Alaska	None	NA	–	NA	NA	–	2005	2013
Arizona	None	NA	–	NA	NA	–	2000	2014
Arkansas	Partial	4/1/97	Ark. Code Ann. § 20-76-409 H.B.1295	NA	NA	–	–	–
California	Partial	7/1/05	AB 1796/Cal. Welf. and Inst. Code § 18901.3	Opted-out	4/1/15	AB 1468 § 49	1992	2014
Colorado	Partial	7/1/97	Colo. Rev. Stat. §§ 26-2-305, 26-2-706	NA	NA	–	2000	2014
Connecticut	Partial	6/18/97	PA 97-2/Conn. Gen. Stat. § 17b-112d	NA	NA	–	–	–
Delaware	Partial	7/17/03	HB 263/Del. Code Ann. tit. 31, § 605	Opted-out	7/1/11	SB 12/31 Del. C. § 512	2009	2014
Florida	Partial	5/30/97	Fla. Stat. Ann. ch. 414.095	NA	NA	–	1996	2014
Georgia	None	NA	–	NA	NA	–	1971	2014
Hawaii	Opted-out	6/16/97	HB No. 480/Haw. Rev. Stat. § 346-53.3	NA	NA	–	–	–
Idaho	Partial	7/1/00	HB 627/Idaho Code § 56-202	NA	NA	–	2008	2012
Illinois	Partial	7/1/97	730 Ill. Comp. Stat 5/1-10	NA	NA	–	1989	2013
Indiana	Partial	7/1/05	SB 523/Ind. Code § 12-20-16-6	NA	NA	–	2002	2014
Iowa	Partial	1/11/97	HF 20/Iowa Code § 239B.5	NA	NA	–	2006	2014
Kansas	Partial	7/1/06	HB 2861/SB 243	NA	NA	–	2011	2014
Kentucky	Partial	7/15/98	Ky. Acts ch. 427, sec. 12/KRS § 205.2005	NA	NA	–	2000	2013
Louisiana	Partial	7/1/97	No. 1351/LSA-R.S. 46:233.2	NA	NA	–	–	–
Maine	Opted-out	4/2/02	H.P. 1665 L.D. 2170/Me. Rev. Stat. Ann. tit. 22, §§ 3104(14), 3762(17)	NA	NA	–	2012	2014
Maryland	Partial	7/1/00	Md. Ann. Code 88A, §§ 50A, 65	Opted-out	10/1/07	Acts 2007, c. 3, §8	2000	2012

Table 1 continued

State	Modification 1			Modification 2			NCRP	
	Type	Date	Bill/law	Type	Date	Bill/law	Start	End
Massachusetts	Partial	12/1/01	2001 MA. Adv. Legis. Serv. 177, § 4400-1000	NA	NA	–	2010	2014
Michigan	Partial	8/18/97	1997 Mich. Pub. Acts 109, § 622	NA	NA	–	1989	2013
Minnesota	Partial	7/1/97	SF 1/MN. Stat. § 256D.024	NA	NA	–	1994	2014
Mississippi	None	NA	–	NA	NA	–	2004	2014
Missouri	Partial	8/28/14	SB 680/MO. Stat. § 208.247	NA	NA	–	2000	2014
Montana	Partial	7/1/05	SB 29/MT. Stat. 53-4-231	NA	NA	–	2010	2014
Nebraska	Partial	5/13/03	LB 667/Neb. Rev.Stat. § 68-1017.02	NA	NA	–	2000	2014
Nevada	Partial	1/1/98	Nev. Rev. Stat § 422.29316	NA	NA	–	2008	2014
(b)								
New Hampshire	Opted-out	8/1/97	N.H. Rev. Stat. Ann. § 167:81-a	NA	NA	–	2011	2014
New Jersey	Partial	11/1/96	No. 15/N.J. Stat. Ann. § 44:10-48	Opted-out	11/1/09	No. 4197/N.J. Stat. Ann. § 44:10-48.1	2003	2013
New Mexico	Opted-out	5/15/02	HB 11/N.M. Stat. Ann. § 27-2B-11(c')	NA	NA	–	2010	2014
New York	Opted-out	8/1/97	N.Y. Laws § 121436	NA	NA	–	1994	2014
North Carolina	Partial	7/1/97	N.C. Gen. Stat. § 108A-25.2	NA	NA	–	1999	2014
North Dakota	None	NA	–	NA	NA	–	2002	2014
Ohio	Opted-out	10/16/09	HB 1/Ohio Rev. Code Ann. § 5101.84	NA	NA	–	2009	2013
Oklahoma	Opted-out	6/13/97	HB 2170/1997 Okla. Sess. Law Serv. Ch. 414	NA	NA	–	2000	2014
Oregon	Opted-out	7/1/97	Or. Rev. Stat. § 411.119 Ch. 581 S.B. No. 825	Partial	8/16/05	Ch. 706 H.B No. 2485 OR ST 411.119	2001	2013
Pennsylvania	Partial	12/23/03	HB 44/62 Pa. Stat. § 405.1(i)	NA	NA	–	2001	2014

Table 1 continued

State	Modification 1			Modification 2			NCRP	
	Type	Date	Bill/law	Type	Date	Bill/law	Start	End
Rhode Island	Opted-out	7/1/04	Family Independence Act Amendment/ R.I. Gen. Laws §§ 40-5.1-8, 40-6-8	NA	NA	–	2004	2014
South Carolina	None	NA	–	NA	NA	–	2000	2014
South Dakota	Opted-out	3/5/09	HB1123/SDCL § 28-12-3	NA	NA	–	2000	2012
Tennessee	Partial	5/14/02	Tenn. Code Ann. §§ 71-3-154, 71-5-308	NA	NA	–	2000	2014
Texas	None	NA	–	NA	NA	–	2005	2014
Utah	Partial	7/4/97	Utah Code Ann. § 35A-3-311	NA	NA	–	2000	2014
Vermont	Opted-out	Unknown	1997 Vt. Laws 61, § 131	NA	NA	–	–	–
Virginia	Partial	3/22/05	§ 63.2-505.2	NA	NA	–	–	–
Washington	Partial	10/1/98	HB 3901/Wash. Rev. Code § 74.08.025	Opted-out	9/1/05	SB 6411/Wash. Rev. Code § 74.08.025	2000	2014
West Virginia	None	NA	–	NA	NA	–	2006	2014
Wisconsin	Partial	10/1/97	Wis. Stat. §§ 49.79, 49.145, 49.148	NA	NA	–	2000	2014
Wyoming	None	NA	–	NA	NA	–	2006	2014

the cutoff, groups are assumed to be characteristically equivalent along observed and unobserved measures.

For our analysis, we used this logic of RD design to identify ban impacts. In this case, treatment is identified on the basis of *conviction date*—felons convicted on or before August 22, 1996 were eligible for benefits upon release and those convicted after were not. The date of conviction acts as the forcing variable and the discontinuity is estimated as the average difference in outcomes for offenders convicted just before and just after August 22.[10] We used prison admission date as a proxy for conviction date because we do not observe actual date of conviction.[11]

[10] A large number of studies have used date/time as an assignment variable modeled within an RD framework. Table 5 in Lee and Lemieux (2010) provides a nice summary of many such studies. Because time is the forcing variable, our approach can also be described as an "event study"—language more common to various social science disciplines.

[11] We argue that prison admission is a good proxy for date of conviction. Prior to conviction, most offenders are housed in jails rather than prisons. After conviction, most offenders are moved to prison quickly.

To be credible, RD analysis requires some assumptions be met. One assumption (mentioned above) is that individuals do not have precise control over their treatment status. In this case, it is to say that offenders (as well as prosecutors, defenders and judges) do not precisely control the timing of conviction. Where this assumption is not met, systematic selection in the timing of drug convictions can threaten validity. Given the power that attorneys and judges possess, we cannot dismiss that possibility that gaming of conviction dates can occur; however, we argue it is unlikely that prosecutorial or sentencing practices were manipulated to systematically favor some drug offenders over others.

To test whether there is any evidence that manipulation in convictions around the date of the cutoff (August 22) occurred, we borrow from an empirical test offered in Jacob et al. (2012). Specifically, we construct two local linear regressions, one to the left of the cutoff and one to the right, that model the percent of sampled drug offenders admitted during each week (as the dependent variable) over time (as the independent variable). We then test whether the intercepts just to the left and just to the right are statistically different from one another. Estimated intercepts and their differences before and after the cutoff are reported in Table 2 for both men and women using a 6-month window of drug offender admissions. Overall these results confirm there is no evidence of systematic manipulation in convictions around the cutoff.

Another assumption of our RD design is that no other changes occurred simultaneously with the timing of the ban that affected recidivism for reasons other than the ban itself. Though we were not able to find any evidence that such a change took place, we cannot directly prove or disprove this condition exists. Instead, we overcome this limitation by incorporating DiD estimation as part of our identification strategy. Specifically, we compared changes around the ban for drug offenders (a group affected by the ban) to similar changes around the ban for nondrug offenders (a group not affected by the ban). In the language of difference-in-differences, estimated impacts within groups (before vs. after) are first differences, and differences in impacts across groups (drug vs. nondrug) are second differences.

The strength of the DiD estimator is that it zeros out bias (in estimated first differences) resulting from unobserved changes also affecting recidivism and closely coinciding with the ban. To accomplish this, DiD identification assumes a constant bias among compared groups such that any unobservable bias impacts groups equally in the absence of treatment (Lechner 2010; Angrist et al. 2009). Thus our application assumes that factors affecting changes in recidivism around the time of, but not as a result of, the ban affect drug and nondrug offenders equally. Traditional DiD models also assume that groups follow similar trends absent the treatment (or "constant trends"); however, because our impacts are estimated as discontinuous jumps (i.e., using RD), assumptions about constant trends are not necessary.

Using this framework, we examined the data in two ways. First, we generated graphical illustrations depicting observed prison return rates for offenders convicted just before and after the ban. Descriptive graphics of this kind are commonly used in regression discontinuity analyses because they can provide useful insights about the nature of the impact being estimated and the strength of the identification. Second, we estimated DiD impacts using Cox-proportional hazards models—models that are well known to the literature on survival estimation (Cameron and Trivedi 2005; Klein and Moeschberger 2003; Allison 2010). We present the equations and discuss the details of our model specification below.

Equations (1) and (2) estimate the probability of reincarceration for offenders released from prison as a function of time at risk. Risk of reincarceration begins on the day an

Table 2 Estimated proportion of sample admitted (weekly) to prison around the cutoff (august 22nd)

	Men			Women		
	Left	Right	Difference	Left	Right	Difference
Pooled	0.019**	0.021**	−0.001	0.017**	0.019**	−0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
CA	0.020**	0.021**	−0.001	0.018**	0.021**	−0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
FL	0.020**	0.021**	−0.002	0.017**	0.016**	0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)
GA	0.017**	0.017**	0.000	0.016**	0.014**	0.002
	(0.002)	(0.002)	(0.002)	(0.005)	(0.004)	(0.006)
IL	0.019**	0.021**	−0.002	0.016**	0.019**	−0.002
	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)
MI	0.016**	0.023**	−0.007*	0.016**	0.026**	−0.010
	(0.002)	(0.002)	(0.003)	(0.005)	(0.005)	(0.006)
MN	0.019**	0.020**	0.000	0.044**	0.046**	−0.002
	(0.004)	(0.004)	(0.005)	(0.015)	(0.009)	(0.018)

Standard errors are reported in parentheses. Stars denote p-values for statistical tests of differences from zero: * indicates a value of 0.05; ** indicates a value <0.01. Numbers are subject to rounding error

offender exits prison, and offenders are followed until a known event occurs or until the end of the data window, at which point the data are right-hand censored. Offenders are followed for as long as the NCRP data currently allow—until December 31, 2014 in most cases.

Both equations share the same specification but are estimated on different samples (drug and nondrug offenders). For drug offenders, we estimate:

$$\lambda_{ij}^{d}\left(t \middle| T_{ij,Pre}, T_{ij,Post}, Ban, M, X_{ij}, C_j\right) = \lambda_0^{d}(t)e^{\left(\beta_1 T_{ij,Pre} + \beta_2 T_{ij,Post} + \tau^{d}(Ban) + \rho^{d}(M) + \pi X_{ij}^{d} + \mu C_j^{d}\right)} \quad (1)$$

Similarly, for nondrug offenders we estimate:

$$\lambda_{ij}^{nd}\left(t \middle| T_{ij,Pre}, T_{ij,Post}, Ban, M, X_{ij}, C_j\right) = \lambda_0^{nd}(t)e^{\left(\alpha_1 T_{ij,Pre} + \alpha_2 T_{ij,Post} + \tau^{nd}(Ban) + \rho^{nd}(M) + \delta X_{ij}^{nd} + \mu C_j^{nd}\right)} \quad (2)$$

In both equations,

$\lambda_{ij}(t)$ is the probability of return to prison for the ith offender from the jth county as a function of time (t) since release from prison; superscript d denotes drug offenders; superscript nd denotes nondrug offenders.

$\lambda_0(t)$ is the baseline hazard function common to all offenders, also a function of time since release; again superscript d indicates drug offenders; nd denotes nondrug offenders.

t is time since prison release, beginning at zero and increasing by one each day an offender is at liberty.

$T_{ij,Pre}$ is the number of days before PRWORA enactment, based on prison admission date for the ith offender from the jth county. Admissions after enactment have a value of zero.

$T_{ij,Post}$ is the number of days after PRWORA enactment, based on prison admission date for the ith offender from the jth county. Admissions before enactment have a value of zero.
Ban is an indicator variable equal to 1 if an offender was admitted to prison after PRWORA.
M is a time-varying covariate for ban modification. It is specified as an indicator variable equal to 1 if an offender is at liberty to fail in a period where a modified ban has been introduced. This variable will only take on a value of 1 if the modified ban was introduced more than a year after PRWORA. Modified bans introduced within a year of PRWORA are characterized as part of the impact of the initial change.
X_{ij} is a vector of individual characteristics for the ith offender from the jth county including age at the time of release from prison, time served in prison and year of release.
C_j is a vector of county-level characteristics for the ith offender from county j, including percentage of households in poverty, median household income, local unemployment, adult population density and high school education.

For these equations: (β_1 and β_2) and (α_1 and α_2) capture the time trends in outcomes before and after the ban for drug offenders and nondrug offenders respectively; τ^d and τ^{nd} represent the treatment effect of the ban (i.e., the first difference) for drug offenders and nondrug offenders respectively; ρ^d and ρ^{nd} represent the average difference in outcomes for drug and nondrug offenders in the modified period; and π, δ and μ capture other baseline differences in offender and community characteristics.

Equation (3) estimates the overall impact of the ban as the difference between estimated treatment effects between groups. For this equation, τ^d and τ^{nd} are defined as before and the second difference, Δ_τ, describes the impact of the ban itself.

$$\Delta_\tau = \hat{\tau}^d - \hat{\tau}^{nd} \tag{3}$$

There are other practical considerations for our estimation. The first is how to identify/label offenders as drug offenders subject to the ban. This is challenging because (1) offenders can be charged with multiple offenses with varying degrees of seriousness; (2) NCRP data only records the top three, most serious offenses; (3) NCRP data do not denote which conviction offenses are felonies and which are misdemeanors, a criterion for the application of the ban; and (4) drug crime admissions can be for revocations (where no new conviction occurs), rather than for new crimes that are subject to the ban (because a conviction does occur).

Given these limitations, we identify (a) drug offender status based on offense type for the first two convicted sentences; and (b) admission status based on the type of admission labeled in the NCRP, i.e., restricting the sample to new court commitments only.[12] We also conducted a sensitivity analysis that defined a drug offender using the first offense only and found results were substantively unchanged. Drug offender status is also carried forward so that, once observed, an offender is labeled a drug offender even when readmitted for a nondrug offense. Nondrug offenders are defined as offenders with no prior conviction for a drug offense.

12 <15% of offenders in our analytic sample are convicted of more than two offenses and, of these, <2% have nondrug offenses for their first two offenses and a drug-related offense for their third offense. Since we cannot know whether this third offense is a felony or misdemeanor, we treat these cases as nondrug offenders.

A second consideration is determining the optimal size of the interval around the cutoff. Larger intervals provide bigger samples for analysis and improve statistical power, but increase the potential for omitted variable bias, especially from poorly specified trends. Conversely, smaller intervals provide the most robust identification but may be too imprecise to reject the null even where true impacts exist. To achieve a balance in light of these tradeoffs, we report estimates across multiple intervals around the cutoff. Specifically, we estimate and compare impacts from four samples of offenders convicted (±) 6 months, 1, 2, and 3 years around the cutoff. This allows us to better judge the overall strength and robustness of our findings.

Tables 3 and 4 report the size of each sample and observed returns to prison for male and female populations (respectively) in each state and for the pooled sample. Drug and nondrug offenders are reported separately. Overall these tables show that most samples are sufficiently sized to detect moderate to large differences in most cases, and small differences in at least some states (particularly in California, Florida, Georgia, Illinois and the pooled sample).[13]

A third consideration is how to estimate impacts on the pooled sample of states. Specifically, estimates can be weighted so that they represent (a) the average impact across individuals or (b) the average impact across states. Each statistic says something different and, without a specific application in mind, it is not clear which one is more interesting from a policy perspective. Estimates giving equal weight to individuals will naturally over represent larger states (such as California) and, in turn, idiosyncratic patterns of practice; however, they will be more precise than estimates weighing states equally. Our solution for this paper is to report both sets of pooled estimates: those weighting individuals equally (shown in Tables 5 and 6) and those weighting states equally (shown in Table 7).

A fourth concern is how to best specify time trends in estimation. More flexible time trends modeled using higher-order polynomials may provide better fits to the data (relative to simple linear trends), but can suffer from overfitting and run a greater risk of introducing bias as a result (Gelman and Imbens 2014). To address this concern, we (1) visually inspected the data to determine the best approach, and (2) tested the sensitivity of our results to alternative specifications. Based on the results of our inspections (presented in “Graphical Analyses”), we adopted an approach using simple linear trends to model the discontinuity, and tested the robustness of these models against quadratic and third-order specifications of time trends.

Finally, an important consideration for estimation is that admission cohorts in two states, Florida and Minnesota, do not extend a full three years (i.e., 36 months) back from August 22, 1996. In Florida, admission cohorts begin January 1, 1996 (6 months before) and in Minnesota they begin January 1, 1994 (30 months before).

The implication for this design is that cohorts of offenders are not evenly observed in the data. Offenders admitted before the start of a term file window are only observed when released during a term file year. In the case of Florida, offenders convicted January 1, 1995 are only observed if they served at least one full year in prison (i.e., released sometime after January 1, 1996). Those serving 364 days (i.e., released December 31, 1995) are unobserved and thus omitted from the sample.

[13] For reference, computations of detectable effects performed in Stata (using the stpower command) show that sample sizes of 200, 500, 3,000, and 10,000 can detect minimum differences in recidivism rates of roughly 0.40, 0.25, 0.10, and 0.05 respectively. These computations assume a two-sided test of a Cox model where the standard deviation of the *Ban/Mod* covariate is 0.5, power is 0.8, and alpha is 0.05.

Table 3 Sample sizes for drug offenders and returns to prison in each state

Sex	Sample	N	Pooled	CA	FL	GA	IL	MI	MN
Men	6-month	Total	32,417	16,331	4778	3032	6692	1226	358
		Returning	18,204	8811	2652	1620	4340	578	203
	1-year	Total	65,617	33,138	9569	6077	13,673	2445	715
		Returning	36,856	17,816	5392	3262	8849	1141	396
	2-year	Total	129,815	65,346	18,426	11,999	27,519	4981	1544
		Returning	72,932	35,191	10,427	6398	17,728	2338	850
	3-year	Total	192,928	95,231	28,113	18,454	41,222	7473	2435
		Returning	108,534	51,328	16,062	9770	26,557	3470	1347
Women	6-month	Total	4474	2468	579	383	885	115	44
		Returning	2325	1357	247	156	503	44	18
	1-year	Total	9201	5053	1269	718	1809	251	101
		Returning	4789	2796	529	297	1035	95	37
	2-year	Total	17,907	9796	2415	1442	3449	565	240
		Returning	9233	5416	998	577	1941	211	90
	3-year	Total	26,270	14,216	3656	2177	5063	817	341
		Returning	13,427	7738	1536	857	2852	307	137

Table 4 Sample sizes for nondrug offenders and returns to prison in each state

Sex	Sample	N	Pooled	CA	FL	GA	IL	MI	MN
Men	6-month	Total	100,984	58,888	13,057	8063	11,839	6574	2563
		Returning	65,702	42,447	6528	4122	7591	3389	1625
	1-year	Total	205,374	120,294	26,102	16,885	23,877	13,076	5140
		Returning	133,638	86,610	13,173	8711	15,319	6603	3222
	2-year	Total	405,179	238,304	48,778	33,277	48,561	26,097	10,162
		Returning	264,425	171,992	24,547	17,107	31,242	13,143	6394
	3-year	Total	599,597	351,432	69,568	51,234	73,000	39,173	15,190
		Returning	390,753	253,498	34,984	26,206	46,938	19,665	9462
Women	6-month	Total	8940	5218	958	1003	1110	481	170
		Returning	5133	3388	354	452	638	216	85
	1-year	Total	17,992	10,541	1962	1917	2276	932	364
		Returning	10,370	6891	732	840	1332	398	177
	2-year	Total	35,423	20,829	3659	3693	4611	1883	748
		Returning	20,467	13,689	1341	1574	2700	804	359
	3-year	Total	52,024	30,469	5226	5524	6787	2912	1106
		Returning	29,920	19,912	1929	2342	3972	1252	513

Given that term file windows in all states overlap the ban implementation date, uneven sample selection is not a fundamental threat to our identification strategy; however, bias may still result if trends in observed recidivism leading up to the ban are not adequately controlled for. As before, graphical illustrations (shown in the "Appendix") provide a

Table 5 Estimated impacts among women using pooled sample

	Window around cutoff							
	6-month		1-year		2-year		3-year	
	Drug	Nondrug	Drug	Nondrug	Drug	Nondrug	Drug	Nondrug
Ban	1.011	0.995	0.996	1.013	1.017	1.010	0.964	0.991
	0.088	0.057	0.060	0.041	0.043	0.029	0.033	0.023
Modification	0.325***	0.296***	0.330***	0.366***	0.373***	0.397***	0.389***	0.421***
	0.055	0.036	0.039	0.030	0.030	0.022	0.025	0.019
% in Poverty	1.007	0.999	1.001	0.996	1.006	0.991*	1.000	0.992*
	0.014	0.008	0.010	0.006	0.007	0.004	0.006	0.004
Median income	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Unemployment	1.028*	1.045***	1.016	1.043***	1.026***	1.044***	1.027***	1.040***
	0.015	0.009	0.010	0.007	0.007	0.005	0.006	0.004
Population density	1.000	1.000	1.000*	1.000	1.000***	1.000	1.000***	1.000
	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Percent of HS educated adults	1.024**	1.012**	1.014**	1.012***	1.013***	1.013***	1.012***	1.013***
	0.008	0.004	0.005	0.003	0.004	0.002	0.003	0.002
Age at release	0.973***	0.984***	0.974***	0.983***	0.976***	0.984***	0.975***	0.983***
	0.003	0.002	0.002	0.001	0.001	<0.001	0.001	<0.001
Time-served	1.000**	0.999***	1.000***	1.000***	1.000***	0.999***	1.000***	1.000***
	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Release year	1.053	1.053	1.003	1.003	1.024	1.024	1.014	1.014
	(0.045)	(0.045)	(0.029)	(0.029)	(0.021)	(0.021)	(0.017)	(0.017)
Pre-trend	0.999	1.000	1.000	1.000	1.000	1.000	1.000**	1.000
	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Post-Trend	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000

Table 5 continued

	Window around cutoff							
	6-month		1-year		2-year		3-year	
	Drug	Nondrug	Drug	Nondrug	Drug	Nondrug	Drug	Nondrug
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	13,177		26,733		52,490		76,818	
Estimated DiD Ban Impact (Δ)	0.016		−0.017		0.007		−0.028	
	0.105		0.073		0.051		0.042	

This table reports coefficients as described in Eqs. (4)–(6), converted into hazard ratios. Standard errors are reported in parentheses. Stars denote *p* values for statistical tests of differences from zero: * indicates a value of 0.05; ** indicates a value of 0.01; *** indicates a value of 0.001. Numbers are subject to rounding error

Table 6 Estimated impacts among men using pooled sample

	Window Around Cutoff							
	6-month		1-year		2-year		3-year	
	Drug	Nondrug	Drug	Nondrug	Drug	Nondrug	Drug	Nondrug
Ban	1.013	0.978	0.996	0.997	0.976	0.998	0.967**	0.985*
	0.031	0.016	0.021	0.011	0.015	0.008	0.012	0.006
Modification	0.382***	0.408***	0.396***	0.413***	0.384***	0.431***	0.397***	0.440***
	0.020	0.013	0.015	0.009	0.010	0.006	0.008	0.005
% in Poverty	0.977***	0.993**	0.989***	0.994***	0.989***	0.993***	0.989***	0.991***
	0.004	0.002	0.003	0.002	0.002	0.001	0.002	<0.001
Median income	1.000***	1.000***	1.000***	1.000***	1.000***	1.000***	1.000***	1.000***
	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Unemployment	1.025***	1.037***	1.022***	1.035***	1.021***	1.033***	1.019***	1.030***
	0.005	0.003	0.004	0.002	0.003	0.001	0.002	0.0011
Population density	1.000**	1.000**	1.000***	1.000**	1.000***	1.000**	1.000***	1.000**
	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Percent of HS educated adults	1.013***	1.020***	1.013***	1.020***	1.010***	1.018***	1.009***	1.016***
	0.002	0.001	0.002	<0.001	0.001	<0.001	<0.001	<0.001
Age at release	0.987***	0.991***	0.987***	0.990***	0.987***	0.990***	0.986***	0.990***
	<0.001	<0.001	0.001	<0.001	<0.001	<0.001	1.000	<0.001
Time-served	1.000*	1.000***	1.000	1.000***	1.000*	1.000***	1.000**	1.000***
	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Release year	1.034**	1.034**	1.021**	1.021**	1.016**	1.016**	1.013**	1.013**
	(0.013)	(0.013)	(0.009)	(0.009)	(0.006)	(0.006)	(0.005)	(0.005)
Pre-trend	1.000**	1.000	1.000	1.000	1.000**	1.000**	1.000***	1.000***
	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Post-trend	1.000	1.000	1.000	1.000	1.000*	1.000	1.000***	1.000*
	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001

Table 6 continued

	Window Around Cutoff							
	6-month		1-year		2-year		3-year	
	Drug	Nondrug	Drug	Nondrug	Drug	Nondrug	Drug	Nondrug
State fixed effects	1.013	0.978	0.996	0.997	0.976	0.998	0.967**	0.985*
N	129,273		262,736		518,791		767,059	
Estimated DiD Ban Impact (Δ)	0.036		−0.001		−0.022		−0.018	
	0.034		0.024		0.017		0.014	

This table reports coefficients as described in Eqs. (4)–(6), converted into hazard ratios. Standard errors are reported in parentheses. Stars denote *p* values for statistical tests of differences from zero: * indicates a value of 0.05; ** indicates a value of 0.01; *** indicates a value of 0.001. Numbers are subject to rounding error

Table 7 Estimated impacts of the initial ban

	Women				Men			
	6-month	1-year	2-year	3-year	6-month	1-year	2-year	3-year
CA	−0.200	−0.135	−0.059	−0.081	−0.013	−0.058	−0.059*	−0.054**
	0.130	0.091	0.064	0.053	0.047	0.033	0.023	0.019
FL	0.553	0.254	−0.013	−0.122	0.099	0.159	0.194*	0.179
	0.358	0.331	0.404	0.524	0.098	0.084	0.086	0.095
GA	0.636	0.196	0.049	0.212	0.007	−0.061	−0.077	−0.090
	0.414	0.279	0.193	0.157	0.120	0.084	0.059	0.048
IL	0.142	0.134	0.020	−0.226	0.127	0.053	0.006	0.072
	0.239	0.170	0.118	0.180	0.076	0.054	0.038	0.048
MI	1.099	0.862	0.501	0.237	−0.135	−0.096	−0.096	−0.102
	0.757	0.493	0.331	0.265	0.190	0.129	0.090	0.074
MN	1.069	−0.019	0.158	0.543	−0.400	−0.212	−0.093	−0.303
	1.187	0.732	0.489	1.23	0.303	0.210	0.150	0.192
State-average	0.550	0.215	0.109	0.116	−0.053	−0.036	−0.065	−0.038
	0.627	0.409	0.307	0.565	0.163	0.115	0.085	0.097

This table reports DiD estimates described in Eq. (3) as differences in hazard ratios. Standard errors are reported in parentheses. Stars denote p values for statistical tests of differences from zero: * indicates a value of 0.05; ** indicates a value of 0.01; *** indicates a value of 0.001. Numbers are subject to rounding error

convenient shorthand to assess differences in recidivism among these "selected sample" periods. Based on the depictions for Florida and Minnesota, we argue that the impact of sample selection appears to be minimal.[14] We conclude that the potential for significant bias in this case is negligible and that our use of a linear trend in pre-periods is a likely sufficient control.

Results

Graphical Analyses

As described earlier, we conducted graphical analyses in advance of formal estimation for the pooled sample as well as for each state. Because the output is lengthy, we present only the results for the pooled sample in the main body of the paper; however, full results are available in the "Appendix". Figure 1 shows illustrations for the pooled sample. Figures in the "Appendix" are identically constructed.

Figure 1 shows four graphs. All four graphs depict changes in recidivism, centered on the timing of (conviction around) the initial ban. The x-axis of each graph denotes the month of the prison admission and the y-axis denotes the percent of offenders returning to prison within a 3-year period after release. For each set of illustrations, the top two illustrations show changes for men and the bottom two show changes for women.

[14] We also conducted empirical tests of impacts excluding Florida and Minnesota. Results from these tests did not change the core findings of this paper.

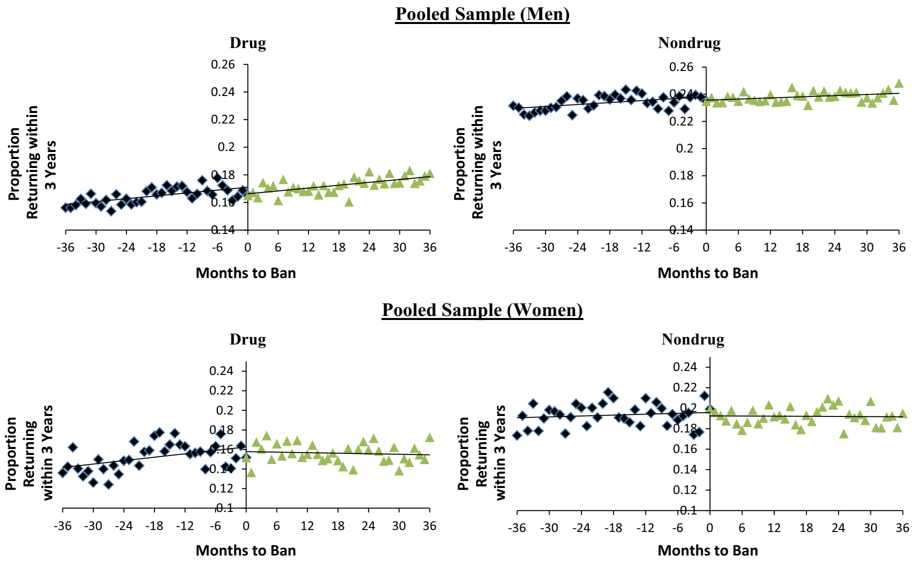

Fig. 1 Illustrations of the proportion drug and nondrug offenders returning to prison within 3 years of release, based on prison admission date centered at Aug 22, 1996

Illustrations on the left show changes for drug offenders and those on the right show changes for nondrug offenders.

Each graph contains two sets of data points (diamond plots and triangle plots) and linear trend lines on either side of the discontinuity for visualization. Comparisons at the discontinuities (i.e., first differences τ and ρ) are derived from shift in these trends at ($x = 0$). Measures of the impact (i.e., second differences Δ_τ and Δ_ρ) are derived from differencing the changes between rows (the top row reports results for drug offenders subject to the bans and the bottom row reports results for nondrug offenders).

Figure 1 shows some interesting patterns. For male drug offenders (top left), a casual interpretation of this picture is that, relative to nondrug offenders (top right), the ban may have caused a small (but perhaps significant) decrease in the rate of 3-year recidivism. However, the picture also shows that a linear trend does not necessarily provide the best fit for these data. Changes over the interval between ($\pm$) 18 months recidivism remain flat; changes on the ends (>18 months and <−18 months) appear to drive the slope of the line. A quadratic estimation of the trend (on the left and right) provides a better visual approximation of the data and a near equivalent statistical fit.[15] Under that specification, the apparent impact of the ban (as a decrease in recidivism around the discontinuity) all but disappears.

For women, rate of recidivism over this entire period is more variable and shows arguably little to no change, especially for nondrug offenders. There do not seem to be any discernable differences in patterns of change between drug and nondrug groups across pre- and post-ban periods.

Similar graphs for individual states (depicting data summarized monthly rather than weekly, shown in the "Appendix") also show consistent patterns of no effect either obvious or systematic. Patterns of recidivism between pre and post periods appear to

[15] The difference in the sum of the squared residuals between these competing trends is <0.0001.

unfold similarly for drug and nondrug offenders and for men and women. In sum, it appears that the introduction of the ban did not have a substantive impact on rates of recidivism for drug offenders either absolutely, or relative to nondrug offenders.

Estimated Impact of the Initial Ban

Tables 5 and 6 summarize the main results of our estimations for women and men respectively. Each table reports results from the full set of regressions for the pooled sample including estimated coefficients for individual- and county-level control variables as well as estimates of τ^{d}, τ^{nd} and Δ_{τ} described in Eqs. (1)–(3). For ease of interpretation, estimated coefficients have been converted to hazard ratios. Standard errors and statistical significant (based on p-values) are also reported in these tables.

Overall these tables show that there is no strong evidence to support the hypothesis that the restriction or denial of benefits for drug offenders, male or female, has led to material impacts in the rate at which offending populations are returning to prison overall. Among women drug offenders (in Table 5), the change in relative risk attributable to the ban is between 0.01 and −0.04. For nondrug offenders estimates are similarly small, between 0.01 and −0.01. Both sets of estimates are robust to the use of both small and large samples around the cutoff and arguably quite stable. Finally, point estimates of the DiD estimator (the last row in each table) show that estimated differences in the ban's impact across groups are not statistically different from zero (between 0.02 and −0.03) for any of the four samples tested.

Table 6 shows results for men. It shows that among male drug offenders, the estimated change in relative risk after the ban is between 0.01 and −0.03. For nondrug offenders estimates are between 0.00 and −0.02. As with women, DiD estimates are quite small (between, 0.04 and −0.02) and no estimates are statistically different from zero.

Finally, given that states themselves are quite heterogeneous in terms of their policies and practices governing correctional populations, we separately estimate and report impacts for individual states. Table 7 summarizes the results of these estimations. Like Tables 5 and 6, Table 7 reports the DiD estimates from each exercise and for multiple samples. It does not report coefficients estimated from individual covariates.[16]

Overall, Table 7 shows results that are consistent with the general findings from the pooled sample. Where samples are small, estimates are highly variable and no consistent findings emerge. Where samples are large, estimates are generally much closer to zero and most states do not show statistically or substantively significant impacts on recidivism. Some exceptions do exist, primarily in California where the largest sample shows a −0.054 decrease in the relative risk for male drug offenders as compared to nondrug offenders.

Additional Tests of the Impact

In addition to the main results above, we conducted additional tests to provide greater context for the results. First, we explored possible associations between the time spent at risk and the impact of restricting benefits. Specifically, we tested whether the ban's impact varies over time-at-risk by separately estimating the average impact of the ban over various segments: (1) during the first 6 months following release, (2) between 6 and 18 months following release, and (3) more than 18 months following release.

[16] Results are available upon request.

Given that the risk of returning to prison naturally diminishes over time (Langen and Levin 2002; Durose et al. 2014), it may be that denying benefits has the greatest impact early in the re-entry period. On the other hand, it may be that benefits impact an offender's ability to address more long-term challenges, especially if it takes time for offenders to begin receiving benefits. If so, then denying benefits may imply consequences later in the re-entry period. In either case, significant, heterogeneous impacts may exist even where the average effect implied is not different from zero.

Table 8 reports DiD estimates of the ban's impact over time-at-risk and across sampling windows, using the pooled sample only. Overall this table suggests some provocative patterns. For both men and women, point estimates of impacts on the relative risk for drug offenders are negative in the first six months after release, though for women, these estimates are not statistically different from zero. However, after 18 months following release, these patterns reverse—point estimates for both men and women are most often positive, suggesting recidivism worsens for drug offenders during this period. Again these patterns are not statistically different from zero; however, they suggest a real possibility that impacts are heterogeneous over time.

From a behavioral perspective, it may not be unreasonable to expect that denying SNAP and TANF benefits has differential impacts in the short run vs. the long run. Consider the following example. If the fear of losing social supports causes short-run behaviors (e.g., more aggressive job searching) among risky offenders to change, the near-term impact of the ban may be beneficial (i.e., by reducing recidivism). At the same time, if the long-term loss of support among less risky offenders creates ongoing hardship, then the net effect over the long term may be detrimental (i.e., by increasing recidivism).

Results from this table do not validate such stories; however, they do provide an impetus for further investigation. Given the inherent limitations in our ITT analysis, we argue that the results from this table are best interpreted as an exploratory exercise and that future investigation should consider the possibility, nature, and causes of heterogeneous impacts of the ban's application.

We performed a second exploratory test of impacts to examine whether there is any evidence to suggest that our null finding (an ITT estimate) would be nonzero if it were estimated as a TOT estimate. This test exploits county-level variation in SNAP participation (as measured in 1995) to compare impacts, but requires two strong (and untestable) assumptions. The first assumption is that offenders coming from counties with higher rates of SNAP participation are themselves more likely to be SNAP/TANF recipients. If

Table 8 Estimated impacts of the initial ban

	Women				Men			
	6-month	1-year	2-year	3-year	6-month	1-year	2-year	3-year
0–6 Months	−0.046	0.010	−0.010	−0.103	−0.017	−0.076*	−0.085***	−0.101***
	0.135	0.093	0.067	0.056	0.046	0.032	0.023	0.019
6–18 Months	−0.065	−0.030	−0.009	−0.031	0.075	0.020	−0.022	−0.008
	0.124	0.087	0.061	0.050	0.042	0.029	0.021	0.017
18+ Months	0.113	−0.024	0.038	0.027	0.042	0.040	0.029	0.037*
	0.122	0.086	0.061	0.050	0.040	0.028	0.020	0.016

This table reports DiD estimates described in Eq. (3) as differences in hazard ratios. Standard errors are reported in parentheses. Stars denote p values for statistical tests of differences from zero: * indicates a value of 0.05; ** indicates a value of 0.01; *** indicates a value of 0.001. Numbers are subject to rounding error offenders coming from high-participation areas are more often themselves recipients of SNAP and/or TANF, then ITT estimates in counties with higher participation come closer to TOT estimates while ITT estimates in low-participation areas are more biased toward zero, since these areas include a higher proportion of noneligible nonparticipants. We looked to see if any such patterns exist.

This test requires information about each offender's county of residence; however, the NCRP does not currently report this information. Instead we relied on the reported county of conviction (an available measure) as a proxy for residence. Thus our test requires a second assumption—offenders are likely to be convicted in the counties in which they reside. While there is no test for this in the NCRP data, there is a known body of literature that supports the argument that offenders often commit crimes near the communities where they live (e.g., Eck and Weisburd 2015; Cohen and Felson 1979; Brantingham and Brantingham 1993, 1995).

Based on these conditions, we examined whether estimated impacts in high-SNAP concentration counties are larger than in low-concentration counties. Specifically, estimates from the pooled regression are grouped into deciles, so that the lowest decile (the 1st) represents the 10% of counties with the lowest proportion of SNAP households (<6% of households), and the highest decile (the 10th) represents the 10% of counties with the highest proportion (>30% of households). Results are reported in Fig. 2.

Figure 2 shows the estimated (DiD) impact of the drug felony ban, by SNAP household concentration, for both men and women using the 3-year sample (results are not sensitive to our sample selection). Overall this figure shows that among both men and women there are no discernable patterns of differences in impacts across counties. In terms of magnitude, average impacts among the bottom 20% of counties are <0.01 different from the top 20% of counties. Overall, the test is weak but suggests no aggregate impacts resulting from the ban.

The third test is a general sensitivity test of our results to alternative specifications that model time trends as higher-order polynomials rather than simple linear trends. Specifically, we tested a quadratic and third-order specification of time trends in addition to the linear arguments shown in earlier regression equations (i.e., three specifications). We did this because more flexible time trends can provide better fits to the data, though some

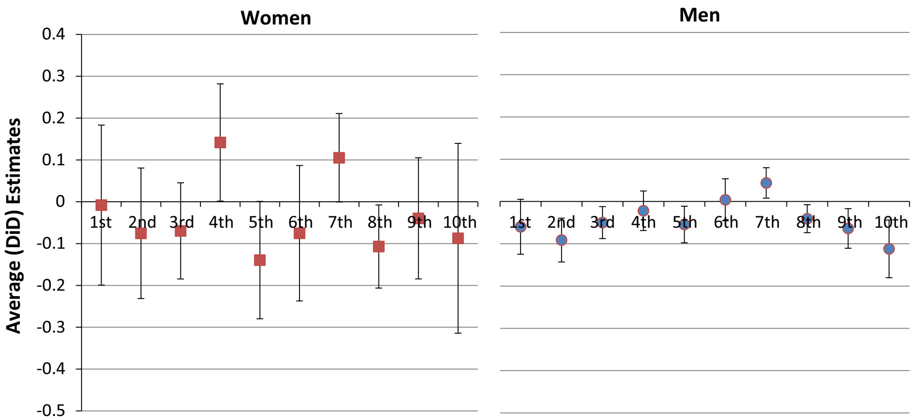

Fig. 2 Average (DiD) Ban impacts across counties in the pooled sample, by SNAP household concentration and sex

literature also argues that higher-order approximations run a greater risk of introducing bias (Gelman and Imbens 2014).

Overall we find that our main results are extremely robust to these alternative specifications. Of the 56 additional tests we conducted for men (across pooled and state samples), 1 test (2%) yielded significant DiD estimates at a p value <0.05. For women, 56 additional tests yielded 2 significant DiD estimates (4% of tests) at a p value <0.05. Given that with so many tests we would expect at least some findings by chance even where no true effects exist, these results reinforce the main finding of no effect.

Finally we tested the sensitivity of our results to our use of the Cox model. While the Cox model allows for flexible estimation of the hazard function, it requires a strong assumption about the proportionality of hazards across measures. By contrast, a less flexible parametric survival model does not impose proportionality. As such, we compared our results from the Cox estimation to a similarly estimated set of parametric survival models that impose a gamma distribution for the survival function. In cases where the model under a gamma distribution could not be estimated, we substituted a Weibull distribution instead. Overall we found that our conclusions are robust to the use of this alternative specification.

Discussion

For the last 20 years, most states have imposed some form of special restrictions on drug felons seeking SNAP and TANF benefits. It seems now though that the social and political tone of the public conversation is at a critical point. Nearly half a century of steady and persistent prison population growth is forcing lawmakers to reconsider the merits of long-standing policies, including the drug felony ban. And despite the political rhetoric, there have been no confirmatory tests of whether and how manifestations of these laws have resulted in tangible differences to prison systems. Without that evidence, it is impossible for policymakers to understand the consequences of actions taken to expand or reduce access to benefits for offenders.

This paper uses a rigorous quasi-experimental approach together with longitudinal prison data and state data on ban adoptions to test competing theories. Results of this analysis are conclusive: neither the denial nor the expansion of SNAP or TANF benefits to drug offenders will adversely or positively impact the size of drug offender populations in state prisons. These results appear to extend to both men and women and to all observed variations of state policies.

The obvious question stemming from this analysis is: “Why don’t welfare laws seem to matter in this context?” There are several potential answers. First, it may be that there are too few offenders receiving benefits to substantively impact prisons and that low rates of recipiency are masking true impacts at the individual level. Our ITT framework is informative, but it does not identify impacts for the subset of eligible offenders.

Second, it may be true that the benefits denied to drug offenders under these laws simply are not significant enough to differentially impact recidivism in a meaningful way. Consider, for example, that the disqualification of an individual from TANF does not disqualify the entire household from benefits. In cases where a drug felon’s household continues to receive benefits, the ban acts to reduce, but not eliminate, benefits. These partial household benefits mitigate the denial of benefits to the individual. Similarly, the PRWORA does not ban drug felons from receiving nonassistance benefits from TANF, including many benefits that we might consider critical to successful re-entry, e.g., services such as counseling, job retention and employment-related services that do not provide basic income support. To the extent that these services have an impact, the ban has less overall influence.

Another possibility is that true heterogeneous impacts among offender subgroups are not observed when estimating the local average treatment effect. We partly explore this possibility in the paper; however, other unexplored offender subgroups may provide additional context. For example, it may be that the impact of the ban is heterogeneous with respect to an offenders' age, race or family size. A deeper examination of possible heterogeneity may reveal new insights.

There are other limitations to our analysis which have implications for the impacts we estimate, some of which might be addressed in the future with improvements to the data or extensions of analyses. For example, identification of offenders in the data is not error-free and has implications for analysis. First, although most offenders in prisons are felons, NCRP data do not allow us to separate misdemeanants from felons and control for them in analysis. To the extent that misdemeanants are part of the sample, impact estimates are biased toward a null finding.

Second, our labeling of offenders as drug or nondrug offenders is not perfect. Since we cannot observe every offender's complete incarceration history in every state, we cannot avoid misclassifying at least some drug offenders as nondrug offenders. Third, our identification of new crimes based on prison admission codes is imperfect. Discretion in the judicial process and variable sentencing structures across states obscure the meaning of these codes (Gaes et al. 2016).[17] Within the constraints of the NCRP, there may be no solution to this problem; however, the integration of new information such as arrest records may help resolve the issue. Similarly, discretion and uncertainty in the use and application of charging offenses makes a more granular investigation of patterns in outcomes relatively uninformative. Arrests records may provide illumination.

Finally, one area that future investigators should explore more closely is whether and how ban modifications have impacted offender outcomes such as recidivism. The identification strategy we present in this paper is not adequate for such an investigation. Credible estimates require more first-hand knowledge (and data) about real-world implementation of modified policies and practices. For example, it is important to know whether offenders released prior to the ban's modification retrospectively seek out benefits for which they have become eligible. Do state agencies actively pursue such cases? How? Answers to such questions are critical to proper sample selection. We leave this exercise to future research.

Overall, there is no doubt that this analysis answers important questions. Will ban removal help to alleviate the problem of mass incarceration? The answer appears to be no. Is there a behavioral basis for denying benefits to drug offenders while providing benefits to nondrug offenders and violent offenders? The answer again appears to be no. However, the answers to these questions also inspire other, yet unresolved questions. What should states do next? Should ban removal be encouraged or discouraged? The answers to these questions are subject to debate; however, that debate should be informed by future research that more closely examines how individuals are directly impacted by the receipt of welfare. Fortunately, BJS is at present undertaking an effort to expand the use of NCRP records by merging them with other offender-level information including welfare use. Use of new data from that effort will undoubtedly lead to novel insights along this line of inquiry.

[17] This paper proposes one alternative construction for a "new crime" using a time-based rule that classifies all prison admissions as new crimes when no release from prison has been observed within the prior year. We test the sensitivity of our results to this classification rule (as well as a 3-year variant of this rule) and find no discernable difference in our findings.

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Appendix

See Fig. 3.

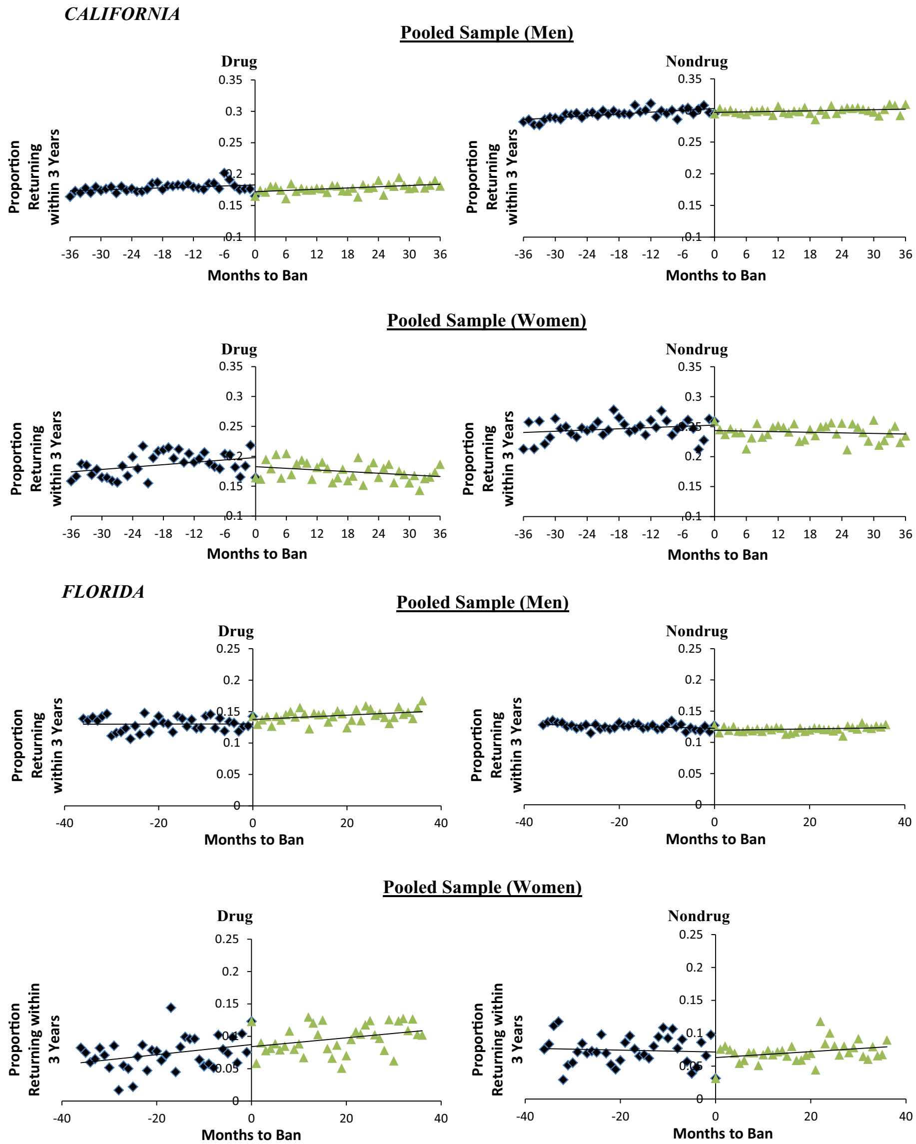

Fig. 3 State-by-state illustrations of the proportion drug and nondrug offenders returning to prison within 3 years of release, based on prison admission date centered at Aug 22, 1996

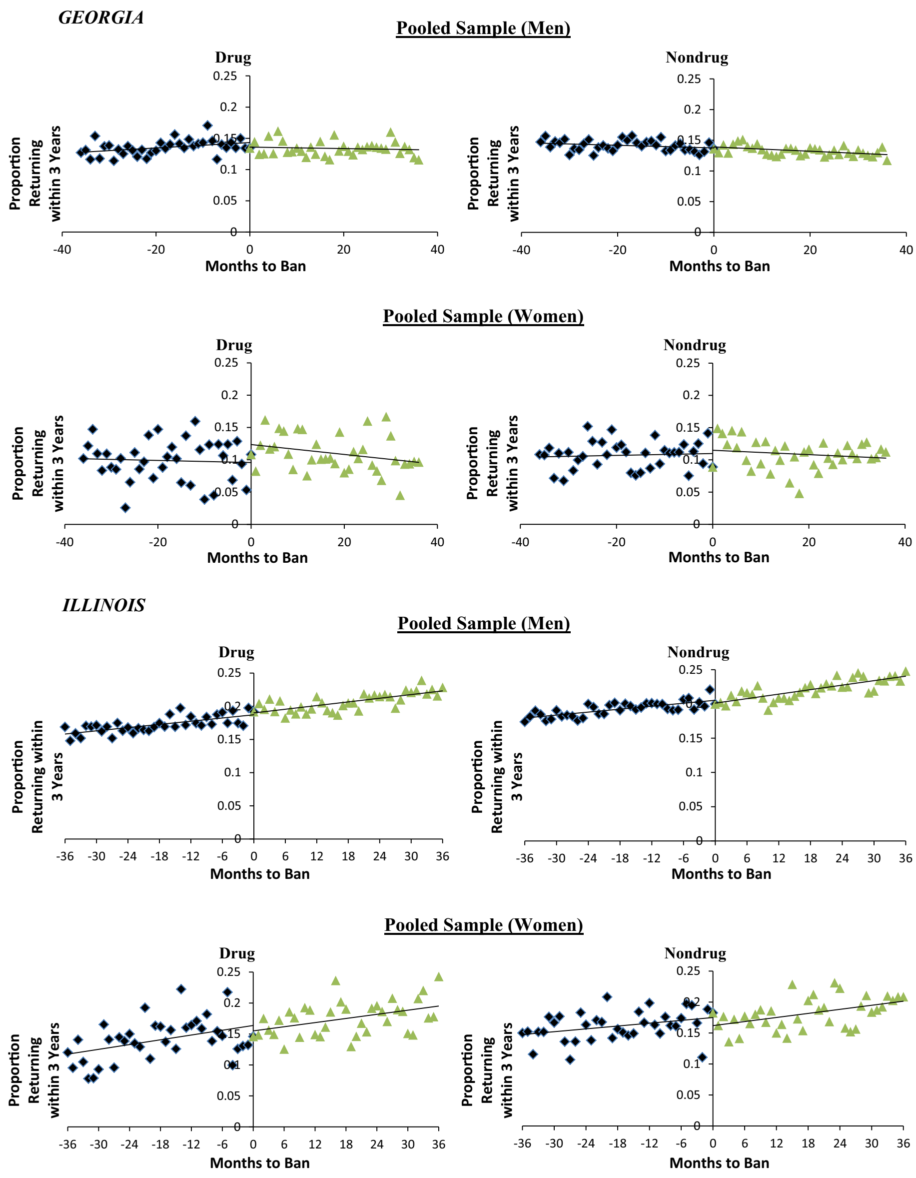

Fig. 3 continued

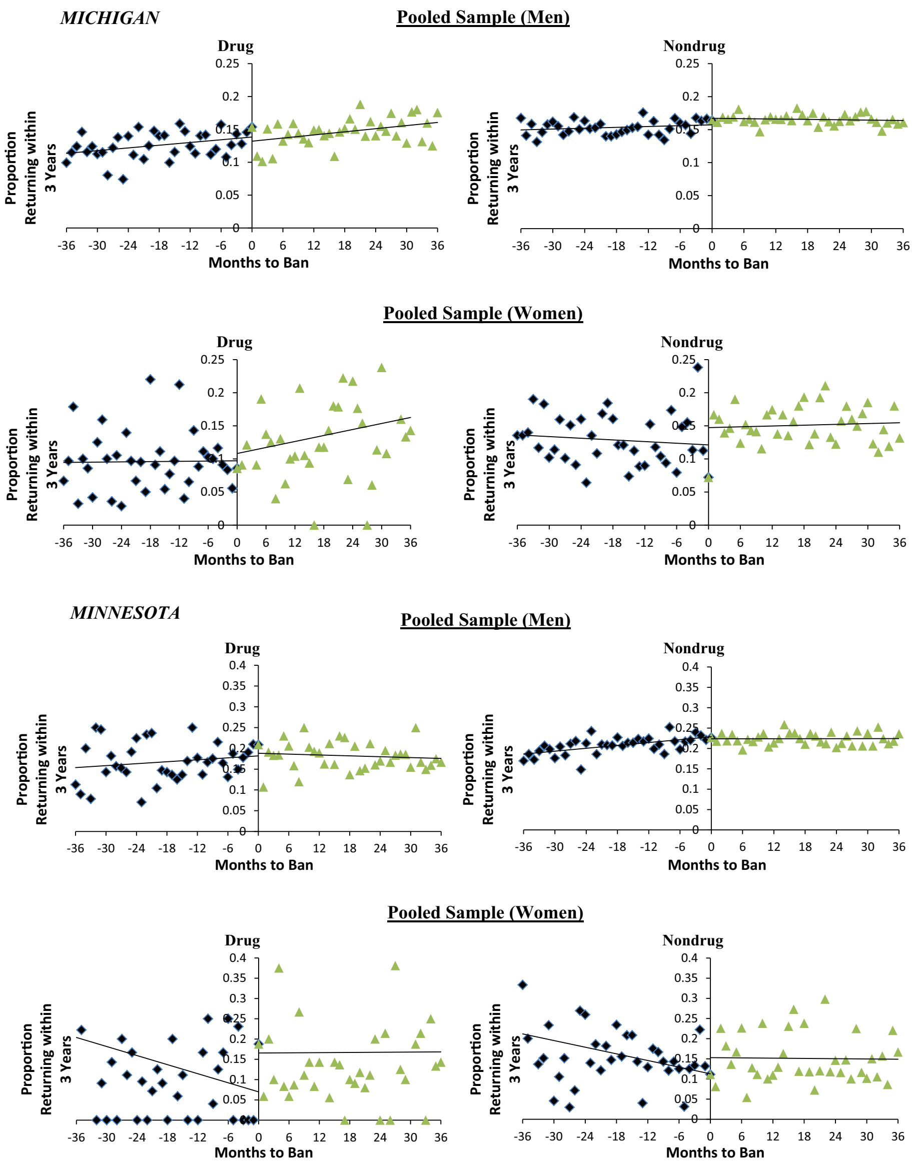

Fig. 3 continued

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